generally adopted; the author of this new "uniform system," however, has chosen the other course.

If the author of the "uniform system" had been contented with tabulating the system of transliteration which has been so long in use, he would have earned the gratitude of those devoted to literature, as well as of those who cultivate science. As it is, I am afraid he has merely given the world of art and letters an opportunity for gibes at what they are sometimes pleased to call the narrowmindedness and pedantry of scientific men.

I may, perhaps, be permitted to give a few examples of the defects of the new system; Г in Russian has three sounds, one nearly resembling the English *g*, another very like *h*, and a third guttural sound, to which there is nothing analogous in our tongue. The author proposes to get over this by transliterating Г by *gh!!* The eminent chemist Hemilian thus becomes masked as Ghemilian, whilst Gustavson appears as Ghustavson, and a well-known political character, Gortchakoff, is altered to Ghorchakovʻ. For comparison, I give these names, and a few others, as transliterated in accordance with the two systems:—

Present system,	New system.
Hemilian	Ghemilian.
Gustavson	Ghustavsonʻ.
Gortchakoff	Ghorchakovʻ.
Alexéeff	Aleksyeevʻ.
Gregoreff	Ghrighorʻevʻ.
Ogloblin	Oghloblin.
Mendeléeff	Mendelyeevʻ.
Chroushtchoff	Khrushchovʻ.
Michael	Mikhail.
Joukovsky	Zhukovskiĕ.

Geographical names are even more weird; for example, it becomes somewhat difficult to recognize under the disguise of Nizhniĭ Novghorod and Volgha, the town of Nijni Novgorod and the River Volga. Such words as "Journal" and "Chemie," when occurring in titles, can be at once recognized; this can scarcely be said of them if the new system of transliteration is used, as they become "zhurnal" and "Khimiĭ" respectively.

It is much to be regretted that the Royal Society, the Linnean Society, and the Geological Society should have pledged themselves to adopt this novel "system of transliteration," instead of adhering to the one which has been so long in use. As a Fellow of the Royal Society, I feel very great regret that the Council are going to adopt this system in their publications, as it will seriously detract from the value of their supplementary "Catalogue of Scientific Papers" now in the press, at all events as far as Russian literature is concerned.

No protest of mine, however, can be half so forcible as the unconscious sarcasm of the author himself, in his paper, where he says that "an expression of grateful thanks is due" to two Russians "who have assisted in the arrangement of the system." The names of the Russians are then given, and if my readers will take the trouble to study them by the light of the table for transliteration by the new system, he will see how they express their appreciation of the author's labours by *carefully avoiding* every one of the novelties he has introduced.

CHARLES E. GROVES,
Editor of the *Journal of the Chemical Society.*
Burlington House, March 17.

HAVING in view the increasing importance of Russian to literary and scientific men, it becomes very desirable to have a uniform system of transliteration, such as that recently proposed in your columns.

But, in order to be useful, everyone must agree to conform to it, nor should any such system be adopted off-hand without full discussion of any points which may seem susceptible of improvement.

It seems to me objectionable to indicate the semi-vowels (ъ and ь) by a simple ʻ, and to omit them altogether at the end of a word. They really correspond, to a certain extent, to our *e* (mute); and I would suggest that it would be better to indicate them by a full letter—perhaps *ĕ* for one and *ê* for the other.

March 11. W. F. KIRBY.

ONE or two points in the criticisms on this subject call for some notice before the publication of a more detailed account of the system.

As regards Mr. Kirby's suggestion, the transliteration of the semi-vowels was discussed, but it was not thought advisable to exaggerate their importance by using two letters for them, especially as their use is becoming discontinued in Russia.

When recommending a uniform system, we did not imagine that Mr. Groves or anyone else would infer that this was intended to limit the right of Russians who dwell in England or who write in English to spell their names as they please; we have not asked Messrs. Kelly to apply it to all Russian names in the Post Office Directory or the Court Guide; we should never think of altering such names in ordinary correspondence. Even in catalogues and records, for which this system is intended, the familiar form should of course be quoted with a cross reference, as recommended by us in the clause dealing with proper names.

Mr. Groves asks why we have not tabulated "the system which has been in use in England for about a century." Our efforts began with an attempt to discover such a system, and resulted in the tabulation of a large number of systems, including that employed by Mr. Groves in the *Journal of the Chemical Society*; since, however, no two authors agree in the English symbols intended to represent either the sounds or letters of Russian words, we endeavoured to frame a system combining as far as possible the features of those already in use in England and America.

We are much obliged to Mr. Groves for supplying further illustrations of the desirability of using *gh* for Г; the letter has, of course, more than the three sounds to which he limits it.

The uniformity of "the system which has been so long in use" may be illustrated by the following examples, in which we confine ourselves to the names of chemists, and to the words quoted by Mr. Groves:—

Consulting the "Imperial Gazetteer," Lippincott's "Gazetteer," and Keith Johnston's "Atlas" alone, we find Nijni, Nijnei, Nishnii, Nizhnee, Nijnii, and Nischnii-Novgorod.

One journal is given in Bolton's "Catalogue of Chemical Journals" as

Zhurnal russkova khimicheskova i fizicheskova;

in the *Geological Record* as

Jurnal rosskoi chimitcheskago i phizitcheskago;

and in Scudder's "Catalogue of Serials" as

Zhurnal; russkoye khimitcheskoye i fizitcheskoye.

Hence it is difficult to see why Nizhniĭ and Zhurnal should be unintelligible.

In the Royal Society Catalogue, the *Geological Record*, and Chemical Society's Journal, the same name is spelt Jeremejew, Jeremejeff, Jereméeff. Which of these words represents the pronunciation?

In the Chemical Society's Journal, Wroblewski and Flawitzky correspond to the Wroblevsky and Flavitzsky of Armstrong and Groves' "Organic Chemistry."

The same journal frequently quotes the name Markownikoff where the same Russian letter (and sound) is denoted both by *w* and *ff*, while in the examples of Mr. Groves it is also represented by *v*; here, of course, and in similar cases, the name comes through a German channel.

Mr. Groves transliterates a few names; since, however, in his "rational" system one Russian letter has more than one English equivalent (*v*, *ff*), and one English letter (*e*) has more than one Russian equivalent, while the sound is not correctly represented (*o*, *é*), it is obvious that this is neither "rational" nor a system (it does not profess to be "empirical"; perhaps Mr. Groves will now call it the "graphic method").

Since, moreover, the system recommended by Mr. Groves is not used by him in the Chemical Society's Journal, we hope that he may yet see his way to adopting the one which has now been accepted by so many of the leading English Societies.

H. A. M.
J. W. G.

"Like to Like"—a Fundamental Principle in Bionomics.

THE following letter has been intrusted to me for seeing through the press, and therefore I deem it desirable to state that it does not constitute the writer's reply to Mr. Wallace's criticism of his paper on "Divergent Evolution." This reply, as previously stated (NATURE, vol. xl. p. 645), will be published by him on some future occasion.

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I cannot allow the present communication to appear in these columns without again recording my conviction that the writer is the most profound of living thinkers upon Darwinian topics, and that the generalizations which have been reached by his twenty years of thought are of more importance to the theory of evolution than any that have been published during the post-Darwinian period. GEORGE J. ROMANES.

London, March 10.

I FOLLOW Prof. Lankester in the use of bionomics to designate the science treating of the relations of species to species. If the theory of evolution is true, bionomics should treat of the origin, not only of species, but of genera, and the higher groups in which the organic world now exists.

In his very suggestive review of "Darwinism," by Mr. A. R. Wallace, in NATURE of October 10, 1889 (p. 566), Prof. Lankester refers to "his (Mr. Wallace's) theory of the importance of the principle of 'like to like' in the segregation of varieties, and the consequent development of new species." Prof. Lankester has here alluded to a principle which I consider more fundamental than natural selection, in that it not only explains whatever influence natural selection has in the formation of new species, but also indicates combinations of causes that may produce new species without the aid of diversity of natural selection. The form of like to like which Mr. Wallace discusses is "the constant preference of animals for their like, even in the case of slightly different varieties of the same species," which is considered not as an independent cause of divergence, but as producing isolation which facilitates the action of natural selection. If he had recognized this principle, which he calls selective association, as capable of producing in one phase of its action sexual and social segregation, and in another phase sexual and social selection, he would perhaps have seen that its power to produce divergence does not depend on its being aided by natural selection.

Mr. Wallace's view is very clearly expressed in the following passages, though I find other passages which lead me to think that the chief reason he does not recognize segregation as the fundamental principle in divergence is that he has not observed its relations to the principle of like to like. He says:—"A great body of facts on the one hand, and some weighty arguments on the other, alike prove that specific characters have been, and could only have been, developed and fixed by natural selection because of their utility" ("Darwinism," p. 142). "Most writers on the subject consider the isolation of a portion of a species a very important factor in the formation of new species, while others maintain it to be absolutely essential. This latter view has arisen from an exaggerated opinion as to the power of intercrossing to keep down any variety or incipient species, and merge it in the parent stock" ("Darwinism," p. 144).

I think we shall reach a more consistent and complete apprehension of the subject by starting with the fundamental laws of heredity, and refusing to admit any assumption that is opposed to these principles, till sufficient reasons have been given. Laws which have been established by thousands of years of experiment in domesticating plants and animals, should be, it seems to me, consistently applied to the general theory of evolution. For example, if in the case of domesticated animals, "it is only by isolation and pure breeding that any specially desired qualities can be increased by selection" (see "Darwinism," p. 99), why is not the same condition equally essential in the formation of natural varieties and species? If in our experiments we find that careful *selection* of divergent variations of one stock does not result in increasingly divergent varieties *unless free crossing between the varieties is prevented*, why should it be considered an exaggeration to hold that in wild species "the power of intercrossing to keep down any variety or incipient species, and merge it in the parent stock," is the same. Experience shows that *segregation, which is the bringing of like to like in groups that are prevented from crossing*, is the fundamental principle in the divergence of the various forms of a given stock, rather than *selection, which is like to like through the prevention of certain forms from propagating;* and I think we introduce confusion, perplexity, and a network of inconsistencies into our exposition of the subject, whenever we assume that the latter is the fundamental factor, and especially when we assume that it can produce divergence without the co-operation of any cause of *segregation* dividing the forms that propagate into two or more groups of similars, or when we assume that segregation and divergence cannot be produced without the aid of diverse forms of selection in the different groups. The theory of divergence through segregation states the principle through which natural selection becomes a factor promoting sometimes the stability and sometimes the transformation of types, but never producing *divergent* transformation except as it co-operates with some form of isolation in producing segregation; and it maintains that, whenever variations whose ancestors have freely intergenerated are from any combination of causes subjected to persistent and cumulative forms of segregation, divergence more or less pronounced must be the result. The laws of heredity on which this principle rests may be given in the three following statements:—

(1) Unlike to unlike, or the removal of segregating influences, is a principle that results either in extinction through failure to propagate, or in the breaking down of divergences through free crossing.

(2) Like to like, when the individuals of each intergenerating group represent the average character of the group, is a principle through which the stability of existing types is promoted.

(3) Like to like, when the individuals of each group represent other than the average character of the group, is a principle through which the transformation of types is effected.

In my paper on "Divergent Evolution" (Linn. Soc. Journ., Zoology, vol. xx. pp. 189–274), I pointed out that sexual and social instincts often conspire together to bring like to like in groups that do not cross, and that in such cases there will be divergence even when there is no diversity of natural selection in the different groups, as, for example, when the different groups occupy the same area, and are guided by the same habits in their use of the environment. There is reason to believe that under such circumstances divergence often arises somewhat in the following way. Local segregation of a partial nature results in some diversity of colour or in some peculiar development of accessory plumes, and through the principle of social segregation, which leads animals to prefer to associate with those whose appearance has become familiar to them, the variation is prevented from being submerged by intercrossing. There next arises a double process of sexual and social selection, whereby both the peculiar external character and the internal instinct that leads those thus characterized to associate together are intensified. The instinct is intensified, because any member of the community that is deficient in the desire to keep with companions of that kind will stray away and fail of breeding with the rest. This process I call social selection. The peculiarity of colour or plumage is preserved and accumulated, because any individual deficient in the characteristic is less likely to succeed in pairing and leaving progeny. This latter process is sexual selection. It can hardly be questioned that both these principles are operative in producing permanent varieties and initial species; and in the circumstances I have supposed, I do not see how the process can be attributed to natural selection. Varieties thus segregated may often develop divergent habits in their use of the environment, resulting in divergent forms of natural selection, and producing additional changes; but so long as their habits of using the environment remain unchanged, their divergencies cannot be due to natural selection.

Mr. Wallace's very interesting section on "Colour as a Means of Recognition," taken in connection with the section on "Selective Association," already referred to, and another on "Sexual Characters due to Natural Selection," offers an explanation of "the curious fact that prominent differences of colour often distinguish species otherwise very closely allied to each other" (p. 226). His exposition differs from mine in that he denies the influence of sexual selection, and attributes the whole process to natural selection, on the ground that "means of easy recognition must be of vital importance" (p. 217). The reasoning, however, seems to me to be defective, because the general necessity for means of easy recognition is taken as equivalent to the necessity for a specialization of recognition marks that shall enable the different varieties to avoid crossing. In the cases I am considering, there is, however, no advantage in the separate breeding of the different varieties, and even in cases where there is such an advantage (as there would be if the variety had habits enabling it to escape from competition with the parent stock, but only partially preventing it from crossing with the same), it does not appear how this advantage can prevent the individual that is defective in the special colouring from following and associating with those that are more clearly marked. The significant part of the process in the development of recognition marks must be in the failure of such individuals to secure mates, which is sexual selection; or in the unwillingness of the

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